last, of whose life we gave some account shortly after. The principal address at the meeting was by Prof. Asa Gray, who sketched Dr. Wyman's life and his work as a biologist. Prof. Gray speaks in very high terms of Dr. Wyman's work. In the memoir on *Troglodytes Gorilla*, read before the Boston Society in 1847, and of which the osteology and introductory history is by Dr. Wyman, and in the subsidiary papers, Prof. Gray says, "may be found the substance of all that has since been brought forward, bearing upon the osteological resemblances and differences between man and apes."

WE note the receipt of the Annual Report for 1873 of the Birmingham Natural History and Microscopical Society, one of the most energetic of this class of societies in the kingdom. There is a very interesting address by the retiring president, Mr. W. R. Hughes, F.L.S., in which he reviews briefly the recent progress of the study of Marine Zoology. We are glad to see that the Society contemplates going so far afield on an exploring excursion as the Mediterranean; our readers may remember that in the autumn of 1872 they made a very successful dredging excursion to Teignmouth. Mr. Hughes suggests that the Birmingham and similar societies should combine in a petition to the proper quarter to obtain any surplus specimens from the *Challenger* collection which may remain after the British Museum and other headquarters for specimens have been supplied. The suggestion seems to us a very reasonable one, though it may be found that after all the *Challenger* specimens will not go very far in this respect. We are glad to see that the Society continues to be increasingly prosperous.

WE are gratified to learn that a Natural History Society and Field Club was successfully inaugurated at Watford on the 23rd ultimo. It has commenced with about fifty members, ladies and gentlemen, and Mr. J. Hopkinson was appointed secretary. We wish the Society every success; it is the only one of the kind in Hertfordshire, and we hope it will set itself in earnest to extend and complete our knowledge of the natural history of that county.

PROF. HAYDEN has lately printed a catalogue of the publications of the United States Geological Survey under his charge, filling a pamphlet of twenty pages.

IN the number of the *Pharmaceutical Journal* for Jan. 23, Mr. E. M. Holmes throws considerable light on the botanical source of the new drug Jaborandi. Prof. Baillon was the first to refer it to a species of *Pilocarpus*, but upon very insufficient materials. Mr. Holmes, however, has succeeded in obtaining better specimens, including some ripe fruits, and from these he arrives at the conclusion that there are two or more distinct varieties of the drug, one of which is very near if not identical with *Pilocarpus pennatifolius*, Lem., another from a species of the same genus not yet known, and another still from a species of *Piper*. These are now in use both in France and England, but several other plants possessing similar properties and known under the same name of Jaborandi are in use in South America. With regard to its physiological action, Mr. Martindale contributes some interesting notes in the *Pharmaceutical Journal* for Jan. 16.

THE *Journal of the Society of Arts* quotes an article from the *Journal de la Société d'Horticulture* on indiarubber-producing plants. This paper is a *résumé* of well-known facts relating to these valuable plants, the only point of interest being in connection with the Central American Caoutchouc Tree, *Castilloa elastica*, Cerv., which, we are told, in the district of St. John, in Nicaragua, furnishes employment to from 600 to 800 persons, in drawing off the juice. In the neighbourhood of Panama about 2,000 persons are so employed.

SOME official correspondence relating to the conservation of the Government forests in Ceylon has been published in Colombo, from which we learn that a good deal of Satin Wood (*Chloroxylon swietenia*), Calamander (*Diospyros quæsita*), and Ebony (*Diospyros ebenum*), exists in the forests, and that the system of felling trees by the natives for firewood and other uses, though illegal, is still carried on to some extent, many of the natives being quite ignorant of forest reservation, while others are such adepts at stealing that the forest officers are not sufficiently numerous to prevent it.

COL. PLAYFAIR, the Consul-General of Algeria, reports that the cultivation of the vine in that country is becoming yearly of greater importance, the advance in the prices of wine in France having given a greater impetus to its cultivation in Algeria. The Sahel, which comprises an area of 125,000 acres, is specially suited to the vine culture, and it is anticipated that this space will some day be nearly covered with the plant. At the time of writing the report, Consul Playfair says, the Phylloxera had not reached Algeria, and the importation of vine-cuttings from any part of Europe was rigorously prohibited.

MR. J. M. WILSON, of Rugby, writes (Jan. 29), with reference to Antares:—

"The subjoined measures may interest the readers of the astronomical column in NATURE, vol. xi. p. 249. I will measure it again soon:—

Position.	Distance.	Date.
268·7	3·46	73·42

THE additions to the Zoological Society's Gardens during the past week include a Clouded Tiger (*Felis macrocelis*) from Burmah, purchased; an Azara's Fox (*Canis azaræ*) from South America, presented by Mr. J. Williamson; a Common Paradoxure (*Paradoxurus typus*), a Bonnet Monkey (*Macacus radiatus*), and a Macaque Monkey (*M. cynomolgus*), all from India, presented by Mr. D. D. Abbott, Miss S. Melley, and Mr. F. G. Lane respectively.

DETERMINATION OF THE VELOCITY OF LIGHT AND OF THE SUN'S PARALLAX*

I HAD the honour to submit to the Academy various improvements relating to the method devised in 1849 by M. Fizeau for the direct determination of the velocity of light. These improvements, tried upon a moderate distance (10,310 metres between the Ecole Polytechnique and Mont Valérien, $V =$ 298,500 kilometres, probable error below 0·01), entirely succeeded, and permitted me to affirm that the improved method was capable of giving results of great precision under the conditions of operating at a greater and better determined distance and employing more powerful apparatus.

The preparations of the expedition for observing the Transit of Venus drew the attention of astronomers to the utility of a precise determination of the velocity of light, for this velocity combined with certain astronomical constants allows the calculation of the sun's parallax, of which the direct observation demands such laborious voyages and the devotion of many astronomers. Thus, at the suggestion of M. Le Verrier, director of the Paris Observatory, and of M. Fizeau, member of the Council, the Council of the Observatory decided at the commencement of 1874 that a determination of the velocity of light should be undertaken without neglecting anything that could give to the operation all the desirable precision.

The Council did me the honour of confiding to me this important operation. Much honoured by and very happy at this decision, I should nevertheless have hesitated to accept so grave a responsibility had I not been strongly encouraged by M. Fizeau, who has not ceased during the whole duration of the labour to offer me the most liberal and precious advice.

After a searching examination of various stations I adopted the Observatory and the tower of Montlhéry, distant about 23 kilometres. I was guided in this choice by the consideration that the value of the distance of these two points is beyond the pale of all discussion. In fact, their position has been determined or verified by the most eminent observers, especially on the occasion of great geodesic works and of the measure of the velocity of sound undertaken by the Academy in the last century, at the time of the meridian operations, of the determination of the metre, of the map of France, and of the new measure of the velocity of sound made by the Bureau des Longitudes. These two stations are thus in a manner classic, and are bound up with the most glorious memories in the history of French science.

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* Translated from a paper read by M. A. Cornu before the Paris Academy of Sciences.

The experiment was installed in conditions worthy of the importance of the problem to be solved. The emission telescope has not less than 8·85 metres focal distance, and 0·37 m. aperture. The mechanism of the toothed wheel permits a velocity of the latter exceeding 1,600 revolutions per second; the chronograph and electric recorder ensure the measurement of time to the thousandth of a second. M. Bréguet, to whom the construction of these pieces of mechanism had been confided, has brought to bear upon their execution that devoted co-operation which he has always given to all the operations with which his name is associated.

All the apparatus is firmly fixed on the superior terrace of the Observatory; an electric communication, establishing the correspondence of the chronograph with the beatings of the pendulum of the meridian chamber, fixes the unit of time with the greatest precision. At the opposite station, on the summit of the Montlhéry tower, there is but a reflecting collimator, of which the objective is 0·15 m. in aperture and 2 m. focal distance; it is surrounded by a large cast-iron pipe, fixed into the wall, in order to secure it from the curiosity of visitors.

The description of the apparatus and of the method of observation will form the subjects of a detailed memoir. I will only recall now the principle of the method. A beam of light is sent across the teeth of the moving wheel, which beam is reflected from the opposite station. The luminous point which results from the return of the rays appears fixed, notwithstanding the interruptions of the beam, owing to the persistence of the impressions upon the retina. The experiment consists in ascertaining the velocity of the toothed wheel, which extinguishes this *luminous echo*. Extinction occurs when, in the time necessary for the light to traverse double the distance of the stations, the wheel has substituted a tooth for the *interval* between two teeth which permitted the passage of the light at starting, so that the extinction of the order n corresponds to the passage of $2n - 1$ semi-teeth during this short space of time. The law of the motion of the mechanism which moves the toothed wheel inscribes itself on a smoked cylinder, and the observer, by an electric signal, records the precise moment when the necessary velocity is attained.

The observations are thus preserved as tracings, which I have the honour to submit to the inspection of the Academy.

The following is a summary of the results obtained from 504 experiments, which I have sought to vary by diversity of wheels, by the number and form of the teeth, as well as by the magnitude and direction of the rotation. These results represent the velocity of light in air expressed in kilometres per second of mean time; they are arranged according to the order n of the extinction which determined them; the number accompanying them represents their *relative weights*, that is, the product of the number of observations into the factor $2n - 1$.

	$n=4$	$n=5$	$n=6$	$n=7$	$n=8$
V	300,130	300,530	300,750	300,820	299,940
$k\times(2n-1)$ ...	15×7	33×9	20×11	10×13	7×15
	$n=9$	$n=10$	$n=11$	$n=12$	$n=13$
V	300,550	300,640	300,350	300,500	300,340
$k\times(2n-1)$ ...	94×17	69×19	72×21	3×23	4×25
	$n=14$	$n=15$	$n=16$	$n=17$	$n=18$
V	300,350	300,290	300,620	300,000	300,150
$k\times(2n-1)$ ...	9×27	65×29	4×31	22×33	35×35
	$n=19$	$n=20$	$n=21$		
V	299,550	,,	300,060		
$k\times(2n-1)$ ...	6×37	,,	36×41		

The agreement of these numbers is as close as can be desired in experiments of such difficulty, and which the least undulation of the atmosphere can hinder; it is true that I always awaited a purity and exceptional calmness of the atmosphere to make these measurements, my patience being thereat much tried, but owing to this precaution the series have always been very regular. It is necessary to add, that in no case can atmospheric disturbances be the cause of systematic errors, for their occurrence is always fortuitous, and on the mean of a large number of observations their influence is *nil*.

The experiments were made at night by means of the Drummond light, with the exception of the series of the fifteenth order, which, by an exceptionally favourable meteorological circumstance, were able to be performed by day with sunlight. Notwithstanding the difference in the nature of the luminous source, the result does not deviate from the mean.

The mean of all these values, having regard to the importance of each group, is equal to 300,330, which, multiplied by the mean refractive index of air (1·0003), gives as definite result the velocity of light *in vacuo*, $V = 300{,}400$ kilometres per second of mean time,* with a probable error below one-thousandth in relative value.

From this the solar parallax is deduced in two different manners.

1. *From the equation of light*.—It is thus that was designated in the last century the time θ which the sun's light takes to traverse the mean radius R of the terrestrial orbit. The reduction of more than a thousand eclipses of Jupiter's satellites gave Delambre $\theta = 473\cdot2$ mean seconds. Calling ϵ the parallax of the sun and ρ the equatorial radius of the earth ($\rho = 6378\cdot233$ km.), we have obviously $R = V\theta$, $\rho = R \operatorname{tang} r$, whence $\operatorname{tang}\epsilon = \frac{\rho}{V\theta}$ and $\epsilon = 8''\cdot878$.

2. *From the aberration of light*.—Bradley, who discovered this phenomenon, found for the annual semi-elongation α of an ideal star situated at the pole of the ecliptic (elongation due to the composition of the mean velocity u of the earth in its orbit with the velocity of light V), the value $\alpha = 20''\cdot25$. According to W. Struve this number ought to be increased to $20''\cdot445$. The equation of condition, designating by T the duration in mean seconds of the sidereal year ($T = 365\cdot26 \times 86400$), will be :—

$$\operatorname{tang}\alpha = \frac{u}{V} = \frac{2\pi R}{V T} = \frac{2\pi\rho}{V T \operatorname{tang}\epsilon}$$

whence

$$\operatorname{tang}\epsilon = \frac{2\pi\rho}{V T \operatorname{tang}\alpha}.$$

By substituting $\alpha = 20''\cdot25$ we deduce $\epsilon = 8''\cdot881$; with $20''\cdot445$ we get $8''\cdot797$. The agreement of the two methods is complete if we adopt Bradley's number.

I will recall the fact that Foucault had, by the method of a revolving mirror, found for the velocity of light the number 298,000 km., but with an indeterminate approximation, and by combining this value with Struve's constant he concluded $8''\cdot86$ to be the value of the solar parallax.

The study of the planetary perturbations leads to a value for the solar parallax which still further increases the interest of this agreement. I will specially cite the profound study of the perturbations of the motions of Venus and Mars made by M. Le Verrier, and which has led to the following numbers: $\epsilon = 8''\cdot853$ by the consideration of the latitudes of Venus at the moments of the transits of 1761 and 1769; $\epsilon = 8''\cdot859$ by the discussion of the meridian observations of Venus in an interval of 106 years; finally, $\epsilon = 8''\cdot866$ deduced from the occultation of ψ Aquarii, observed by Richer, Picard, and Rœmer on the 1st of October, 1672; the mean of these values gives $8''\cdot86$.

To summarise, the methods which serve in astronomy to determine the parallax of the sun can be classed into three groups :—

1. *Physical methods*, founded on the observation of an optical phenomenon; they comprise the observation of the eclipses of Jupiter's satellites, or the aberration of the fixed stars, combined with the value for the velocity of light deduced without the intervention of other astronomical phenomena; the present work permits us to profit by the observations which are the basis of the method: the results are, $\epsilon = 8''\cdot88$, $8''\cdot88$, $8''\cdot80$. Mean, $8''\cdot85$.

2. *Analytical methods*, which depend on the comparison of astronomical observations with theoretical laws founded on the principle of universal gravitation: they give, as we have just seen, values near $8''\cdot86$.

3. *Purely geometrical methods*, depending on the parallactic displacements of the planets near the earth: the oppositions of Mars furnished, in 1862, $\epsilon = 8''\cdot84$. But the transit of Venus across the sun is the phenomenon in which the geometrical method can attain the greatest precision.

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* The velocity in English miles per mean second will be 186,700.

Thus we see what interest there is for astronomy to determine the parallax of the sun by three independent methods; I trust that the experiments that I have the honour to submit to the Academy will justify, by their precision, the theoretical importance of the physical method. R. M.

ON THE MUSCULAR MECHANICAL WORK DONE BEFORE EXHAUSTION*

II.

ONE of the principal sources of error in the series of experiments before discussed, was the fatigue caused by the downward-plunging weight. To eliminate this, the apparatus shown in vertical section in Figs. 1 and 2 was constructed. The shelf *o*, armed with a plate of car-spring caoutchouc, serves as a support for the weight. This shelf is fastened by the iron bands, *I*, and a vertical backpiece, *B*, to the slide, *u*, being further supported by a crosspiece. By suitable blocks, *a*, the slide may be raised to any desired height, which height is read off on a scale marked on the upright pieces, *A*. *s* is a support for the arm during the interval of rest, and it can be adjusted to any desired height; *W* is a wire, from which a small cord, *b*, passes horizontally to the wall of the room. By some modifications this cord can also be made movable, which will become necessary when I come to investigate the influence of elevation of the arm, upon the work done. The weight is a bucket of shot, provided with a stiff bail and a wooden handle, so that for any position of the arm while lifting the weight, the line passing through the centre of the hand and the centre of gravity of the weight is a vertical. Placing the bucket upon the shelf *o*, the experimenter stands to the right of the apparatus (as in Fig. 2), and lifts the weight until his knuckles touch the cord *b*. The instant of beginning and close of this interval of work is marked by the sharp click of a metronome, the time of whose beat is *t*. At the instant when the knuckles touch the string, the weight is grasped by an assistant, † and by him lowered to the shelf *o*, the arm of the experimenter being entirely relaxed, and resting upon the stiff bail of the bucket and the support *s*. ‡ This is continued until the arm becomes unable to lift the weight to the required height. The determination of the number of lifts should *never* be made by the experimenter, who should furthermore try to lose all estimate of time during the process. In the earlier experiments it became evident that the arm not only grew gradually stronger, but also that it varied greatly from day to day. In order to get some measure of the strength, the arm was exhausted on each day of experiment, by a constant weight (5·0 kilos.) lifted through a height $h = 0{\cdot}70$ metres in a time $\tau = 1{\cdot}25$ sec. The values of n were all reduced to the mean strength, as shown by the constant experiment.

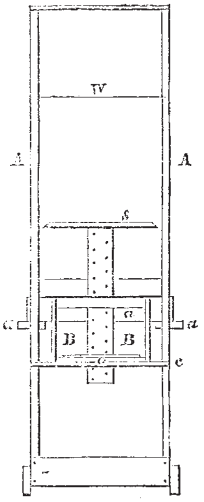

FIG. 1.

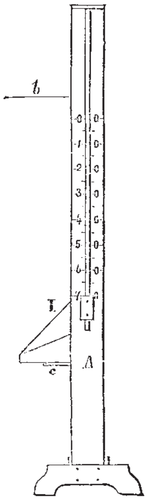

FIG. 2.

In the series here given, w was variable, $h = 0{\cdot}70$ metres, and $\tau = 1{\cdot}25$ sec., the interval of rest being equal to the interval of work. The mean value of n for the constant experiment for the weights $w = 3{\cdot}0$, $3{\cdot}5$, $4{\cdot}0$, &c.—$7{\cdot}5$, in all 100 experiments, is 35·79. Taking these values, c, as the measure of the strength, and assuming that the work done with any weight at different times is proportional to the strength,* and we have—calling n' the number of lifts before exhaustion, and n the number reduced to the basis of the mean constant (35·79)—

$$n = \frac{35{\cdot}79}{c}\, n'$$

from which we have the following values of n, which hereafter we shall call n (obs.) Each of these values is a mean of ten independent determinations.

TABLE II.

w	n (obs.)	n (calc.)	$d\,n$	e
2·50	283	242	− 14·4	7·5
3·00	152·5	150·3	− 1·4	3·7
3·50	95·8	99·4	+ 3·6	3·6
4·00	67·2	69·2	+ 2·9	2·9
4·50	51·2	50·1	− 2·1	3·3
5·00	36·9	37·4	+ 1·3	2·4
5·50	28·6	28·7	+ 0·3	2·0
6·00	22·7	22·5	− 0·9	1·3
6·50	18·1	18·0	− 0·5	1·1
7·00	14·5	14·6	+ 0·7	0·7
7·50	10·4	11·9	+ 14·4	0·9
8·00	7·7	9·9	+ 28·5	5·2

The determination of n for $w = 7{\cdot}5$ and $8{\cdot}0$ was consciously bad, as the arm was unable to manage such weights at such a velocity, so that I was obliged to stop *before the arm was exhausted*.† The values of n for w less than 3·0 were also dropped in the final calculation, as with such light weights the work is found to vary greatly with a slight variation of strength.

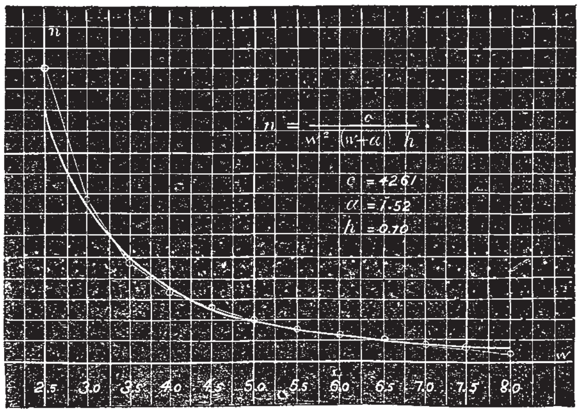

FIG. 3.

Assuming the arm to be a uniform cylinder, and denoting by a one half the weight of the arm, and we have as the dynamical work done before exhaustion—

$$W = (w + a)\, h\, n \qquad (6).$$

The value of a can be determined directly by means of a spring-balance. Exhaust the arm thoroughly, then grasp the hook of the spring-balance, the dial of which should be turned from the face of the experimenter, the reading being done by an assistant. After several minutes the muscles tire, and the practised experimenter can then gradually relax them fully. Untrained muscles, when thus tried, act involuntarily, and precise

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* Continued from p. 257.

† I am under many obligations to friends, among whom I may mention Mr. W. C. Preston and Mr. D. A. Myers, for aid in this very laborious work.

‡ The arm is raised in the plane which make an angle of 45° with the vertical plane passing through the centres of the shoulder-joints.

* This is only approximately true, but is accurate enough for our purpose. We shall develop this point further on.

† This is a highly important point. Try to lift 20 kilos. in a second of time through 0·7 metres. You will fail to lift it once, and *yet not be exhausted*. The problem of maximum velocity attainable with different weights, is wholly different from the one under consideration. I think Mr. Haughton has overlooked this influence on his own experiments.